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valleys by the measure of some multiplier much greater than unity. When valleys are irregular the basal retardation is still further increased and the movement of the ice is correspondingly transferred to the upper horizons. The irregularities of the coast of the region in question give this fact special application. The interpretation of the author is, therefore, quite consistent with a *limited* extension of the ice border, but quite inconsistent with profound extension. The whole of the phenomena described in the paper are precisely concordant with moderate extension. They are as precisely discordant with great extension.

The remainder of the paper consists of a description of the Cornell glacier, of the evidences and amount of former invasion, of the recent advance and retreat of the ice, and of the evidences of present retreat of the Cornell glacier. This portion embraces much valuable data, unless it is vitiated, as it probably is not, by the lack of care which marks the controversial part. It is accompanied by excellent photographs, all of which, as the writer would interpret them, show evidences of greater or less glacial modification of contour.

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SUGGESTIONS FOR A NEW METHOD OF DIS-
CRIMINATING BETWEEN SPECIES AND
SUBSPECIES.

ACCORDING to present usage the rule which determines whether a particular animal or plant shall stand in our books as a species or subspecies may be stated as follows: *Forms known to intergrade, no matter how different, must be treated as subspecies and bear trinomial names; forms not known to intergrade, no matter how closely related, must be treated as full species and bear binomial names.* This principle was first distinctly formulated in the Code of Nomenclature of the American Ornithologists' Union, published

in 1886. In the remarks that follow, the authors of the Code state: "The kind or quality, not the degree or quantity, of difference of one organism from another determines its fitness to be named trinomially rather than binomially. A difference, however little, that is reasonably constant, and therefore 'specific' in a proper sense, may be fully signalized by the binomial method. Another difference, however great in its extreme manifestation, that is found to lessen and disappear when specimens from large geographical areas or from contiguous faunal regions are compared is, therefore, not 'specific,' and, therefore, is to be provided for by some other method than that which formally recognizes 'species' as the ultimate factors in zoological classification. In a word, *intergradation* is the touchstone of trinomialism."

Eleven years have now elapsed since the publication of the A. O. U. Code of Nomenclature, in which the above canon and statement were first published. During this period the plan advocated has been very thoroughly tested, not only by ornithologists, but by systematists in many other departments of zoology, and also in botany. The time has come, therefore, when it should be possible to examine its practical workings, with a view to ascertaining whether or not the system is satisfactory.

In practice it has been found that only in a small percentage of cases does an author have at his command a sufficiently large series of specimens, from a sufficient number of well-selected localities, to enable him to say positively that related forms do or do not intergrade. The result of this obvious embarrassment is that authors usually exercise their individual judgment as to the probable existence or non-existence of intergradation, thus introducing the personal equation it was hoped to avoid. The natural result is a degree of inconsistency in the use of trinomials which has formed the subject

of much criticism, and which, under existing rules, it seems impossible to avoid. But its inconsistencies are not the only objection to the present system. From the nature of the case, increase in knowledge as to the interrelations of forms often shows that those treated as full species really intergrade, and that closely related forms supposed to intergrade really remain distinct, necessitating corresponding changes from a binomial to a trinomial, and *vice versa*. Changes of this kind may be found in the A. O. U. Check-List of North American Birds by comparing the editions thus far issued.

It will be many years, even in America, before it will be possible to say that certain forms do or do not intergrade, and until that time a fixed nomenclature will be impossible.

In view of the objections to the present system—incurable inconsistency, inevitable changes with increase of knowledge, and consequent delay in attaining a fixed nomenclature—and also in view of what to me seems the logic of the case, it would appear desirable to modify the system in the interest of consistency, stability and common sense.

In systematic zoology and botany a knowledge of the *degree of difference* between related forms is infinitely more important than a knowledge of whether or not the intermediate links connecting such forms happen to be living or extinct. It would seem, therefore, since it is impossible for our nomenclature to tell everything we wish to know about a species, that it would serve a more useful purpose if the terms species and subspecies were so used as to indicate degree of difference, rather than the author's opinion as to the existence or non-existence of intergrades. It may be argued that 'degree of difference' is an elastic term, incapable of measurement and subject to the same personal equation that besets the present system. While this is to a certain

extent true—since authors rarely see objects through the same spectacles—it is also true that individual opinion as to whether or not an observed degree of difference is worthy of specific recognition would vary within much narrower bounds than in the alternative case of hypothetical intergradation; and, further, that the change in nomenclature incident to the discovery of new facts, inevitable under the old system, would be entirely done away with.

This leads to what is, after all, the most practical consideration in connection with the proposal to be governed by *degree of differentiation rather than intergradation* in our choice of binomial and trinomial nomenclature, namely, the quantity of difference it is desirable to accept as a measure of specific distinctness. Some authors, like Mr. Lydekker in England, and Mr. Roosevelt in this country, would have us limit the number of species to types of groups, many of which are commonly regarded by naturalists as of subgeneric or even generic weight. Among the larger mammals their species are nearly always used in a super-specific sense. Thus they would have one large wolf, one small wolf, one black bear, one large brown bear, and so on, urging that the recognition of a number of related species is inconvenient, interfering with the clear and easy comprehension of the different groups. Of course this is true, but since the function of the naturalist is neither to create nor destroy species, but to recognize, describe and learn as much as he can about those which nature has established, a difficulty arises in carrying out their views of classification. It is one thing to say—without taking the trouble to find out the characters that distinguish a batch of species—what one thinks ought to be done for the easier comprehension of the science; a very different thing to arrange the animals themselves in accordance with the species which actually exist.

A good deal, of course, depends on the point of view. Mr. Lydekker as a paleontologist and compiler of excellent general works on natural history, and Mr. Roosevelt as a hunter and writer of the best accounts we have ever had of the habits of our larger mammals, find it inconvenient and annoying to be confronted by a large number of species. Still, if we examine the writings of these authors closely it becomes evident that they usually accept without complaint such species as have been currently recognized by their predecessors. This is only human nature, for are we not always more ready to challenge the announcement of new facts than to suspect those with which we have been long familiar?

In my judgment, forms which differ only slightly should rank as subspecies even if known not to intergrade, while forms which differ in definite, constant and easily recognized characters should rank as species even if known to intergrade.

In a recent article in SCIENCE, Mr. Roosevelt protests against the use of the word species where "it has entirely different weights in different cases," and cites examples of what he considers its proper use. But he forgets a host of cases in which admittedly distinct species are not separated by any such gaps as those he mentions. Mr. Roosevelt, in addition to being a good deal of a mammalogist, is something of an ornithologist, and has made contributions of value to ornithological literature. He knows, therefore, that in the eastern United States we have two species of falcons belonging to the genus *Falco*, the Sparrow Hawk and the Duck Hawk, and two species of woodpeckers belonging to the genus *Melanerpes*, the Red-headed Woodpecker and the Red-bellied Woodpecker, in both of which cases the species are separated by the kind of gaps he likes. He knows also that we have two

species of thrushes, the Olive-back and Alice's, and two species of small flycatchers, Traill's and the Least, in both of which cases the species are so much alike that a trained eye is necessary to tell them apart. What will he have us do with these birds? Shall we unite the two thrushes and the two flycatchers? If not, how can he reconcile his theory to the enormous difference in weight of characters that distinguish the species of hawks and woodpeckers, contrasted with those that distinguish the thrushes and flycatches? The real difficulty is that in nature some existing species are closely related, while others are widely separated. Still, suppose for the sake of argument that we do attempt to carry out Mr. Roosevelt's suggestion to lessen the number of species by uniting some of those that are more or less closely related, and suppose we select for this purpose two groups of mammals—the bears and coyotes—against whose species he has developed such a violent aversion. If in case of the bears we try to get rid of either the Grizzly (*Ursus horribilis*), the Barren-ground (*U. richardsoni*), the Yakutat bear (*U. dalli*), or the huge Alaska Peninsula bear (*U. middendorffi*), and in the case of the coyotes we aim to abolish any one of half a dozen species, as the northeastern *Canis latrans*, the California *C. ochropus*, the Rio Grande *C. microdon* or the Mexican *C. cagottis*, we are at once confronted by the same difficulties that would beset Mr. Roosevelt were he to undertake to unite under a smaller number of specific names such birds as the Hermit, Wood, Olive-back, Bicknell's and Wilson's thrushes, or the Warbling, Red-eyed, White-eyed, Hutton's and Philadelphia vireos. These difficulties are of several kinds and involve the solution of such questions as: (1) How many and what species shall be selected as the favored ones with which the others shall be merged? (2) Which of the species to be

distributed among the chosen ones shall go to this and which to that? (3) When we have made what seems to be the best conglomeration practicable with the material at hand, how are we to frame descriptions that will cover such incongruous assemblages and distinguish them from one another? And after all (4) why should we try to unite different species under common names? It is well to remember that the book of nature is not always easy to read, and that in the great majority of cases we know nothing of the ancestry of individual species.

A prolific source of error respecting the interrelations of allied forms is the common assumption that such forms are necessarily derived from one another. In numerous instances this is not the case, their origin dating back to a common ancestor now extinct. Thus a species which in Pleistocene times had a transcontinental distribution may have given off in remote parts of its range several lines of descendants, each of which has since spread over so large an area that the resulting forms, originally widely separated, have now come to inhabit contiguous areas and as a consequence are assumed to intergrade.

Possibly the skepticism of Mr. Lydekker and Mr. Roosevelt as to the validity of the new species of mammals recently described is a result of unconsciously overlooking the wide difference in the present status of the sciences of ornithology and mammalogy. Relatively, ornithology is a finished science, while mammalogy is yet in its infancy. Birds have been studied by scores of able naturalists; mammals by comparatively few individuals. The disproportion in available material is even greater, for museums containing many thousands of bird skins rarely have more than a few hundred mammals. Until recently our museums have made no effort to secure series of mammals from extreme points in their

geographic ranges, so that specimens might be placed side by side for direct comparison in order to ascertain positively—instead of assuming theoretically—what the differences really are. Even to-day no museum in the world possesses anything like an adequate series of any of our larger mammalia. In the few cases in which specimens of supposed single species have been brought together from widely separated areas it has generally been discovered that two or more species had been confounded under a single name.

In America the science of mammalogy took a long sleep after the pioneer work of Audubon, Bachman and Baird, which ended with the publication of their great works in 1854 and 1857. From this time until about ten years ago little advance was made. Then an active interest in the subject sprang up and scientific collecting really began. It is probably safe to say that during the past decade more mammals have been collected in North America alone than were previously contained in all the museums of the world. Furthermore, these specimens are not only of infinitely better quality than the old, but are accompanied by full data, uniform field measurements and perfect skulls. As a result, it is now becoming possible, for the first time in the history of the science, to bring together for actual comparison series of specimens in the different groups covering the greater part of the range of these groups in an entire continent. Is it surprising that the study of such material should result in the discovery of many new species? As a matter of fact, during the last ten years the number of species known in North America has been considerably more than doubled, and several entirely new genera have been found.

In criticising a recent paper of mine on the coyotes, Mr. Roosevelt says: "The important point is the essential likeness of all the coyotes one to the other, and their

essential difference from the big wolves with which they are associated, and which are themselves essentially like the big wolves of Europe and north Asia; and it seems to me that these facts can best be brought out by including the coyote and the wolf in one genus and treating each as a species. Then the geographical and other varieties may or may not be treated as worthy of subspecific rank according to the exigencies of the particular case."

The above remarks are based on a total misapprehension of the facts in the case and remind one of the judge who gave his decision first and tried the case afterward. As a matter of fact, two assumptions are made by Mr. Roosevelt which are widely at variance with the facts. The first is the assumed 'essential likeness of all the coyotes one to the other;' the second, the assumed 'essential difference [of the coyotes] from the big wolves.' I can show Mr. Roosevelt a series of skulls of wolves from the United States in which the great gaps are not between the big wolves and coyotes, but between two species of big wolves and two of coyotes. Thus, there is an enormous gap between the large northern coyote (*C. latrans*) and the small *C. microdon* from the lower Rio Grande, and another great gap between the big red wolf of Arizona and the big gray wolf of Wyoming. On the other hand, no such gap exists between the northern coyote and the big red wolf of Arizona, the skulls and molar teeth of these species resembling one another surprisingly. Mr. Roosevelt's third assumption, the assumed essential likeness of our big wolves to the big wolves of Europe, may be correct or incorrect according to the parts of Europe and America from which specimens are taken. The southern wolves of the two countries are too unlike to require close comparison, and even in the case of the northern forms the specific distinctness is apparent as soon as the skulls are brought together. Thus,

not to mention other differences, the long muzzle and narrow forehead of the wolf of our northern plains offer a sufficient contrast to the short muzzle and broad forehead of the Scandinavian animal.

In my paper on the coyotes eleven forms were recognized, of which seven were named for the first time. All were treated binomially, but it was intimated that *pallidus* and *lestes* would probably be found to intergrade with *latrans*, and that *estor* might intergrade with *mearnsi*, leaving eight as distinct species. It was stated that the available material was insufficient to admit 'of determining which members of each group do and which do not intergrade,' for which reason it was necessary, in obedience to the rule respecting the use of specific and subspecific names given at the beginning of this article, to treat all as species. This was done reluctantly and with the conviction that the rule is illogical and should be changed. If the plan here recommended is adopted we need not care whether intergradation occurs or not, but may bring together as subspecies the closely related forms, and accept as species those more distantly connected.

In conclusion, let me appeal to museums, sportsmen and naturalists to take advantage of every opportunity, before it is too late, to secure and preserve specimens of our larger mammals from remote parts of their ranges. In Europe it is certain that many species have been exterminated through the agency of man, and in this country the process is not only about to be repeated, but has already begun. The familiar story of the vanishing buffalo is only one of many. The largest carnivorous animal of the United States, the giant grizzly of southern California, is on the verge of extinction, and it is doubtful if a museum specimen will ever be obtained. The large wolves have been exterminated over more than half the area they formerly

possessed, and no one knows what forms have disappeared and an unknown form of elk or wapiti which within the memory of our fathers—and of some men still living—inhabited the Alleghany region from North Carolina to the Adirondacks has been wiped off the face of the earth.

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ON THE VARIATION OF LATITUDE.*

At the autumn meeting of the Academy in 1894 the author had presented the numerical theory of the motion of the pole, synthetically derived from the observations from the beginning of the history of the astronomy of precision up to that time, in its complete development, exactly as it stands to-day. Since then he had been interested to compare it with the various series of observations subsequently published, not only for the purpose of verification and improvement of the numerical values of the various constants, but also to detect any additional characteristics which these later data might make apparent. These additional investigations had individually been neither extensive nor important enough to call for separate publication; since their general result has been merely a satisfactory confirmation of the previous deductions as to the nature of the law of these motions, without furnishing material improvement of the numerical elements. But sufficient material has thus been gradually accumulated to make the present communication of some interest.

The new material to be here utilized consists of the various series of observations by Tallcott's method up to the middle of 1896, so far as published, at the following European stations, named in order of longitude: Kasan, Vienna, Prague, Berlin, Potsdam, Karlsruhe and Strassburg. In Amer-

*Abstract of a paper presented by Dr. S. C. Chandler.

ica we have Doolittle's series at Bethlehem, which was brought to a close in the summer of 1895. He is now carrying forward a new series at Philadelphia of which we may hope soon to see the results. Of the series at Columbia University, by Rees, Jacoby and Davis, begun in the spring of 1893 and still current, there have come to hand the results for the first fourteen months. It is an extremely fortunate circumstance that a portion of this series, yet unreduced, will bridge the gap in Doolittle's work rendered unavoidable by his removal from Lehigh University to the University of Pennsylvania.

The curves of latitude variation from these various series were then exhibited, and compared with the numerical theory. This comparison shows a fidelity of representation eminently satisfactory, the differences between computation and observation being practically within the range of errors of observation.

A determination of the elements of the ellipse of the annual component of the polar motion was then presented, made from the newer observations, independently of the older ones previously used. The resulting elements are practically identical as to form, size and position of this ellipse. This seems to show that the axis of this elongated vibratory motion is stationary on the earth's surface, along a meridian forty-five degrees east of Greenwich. This negative evidence as to any apsidal motion seems to be of extreme importance in its bearing on the theory of the earth's rotation.

A demonstration was then presented of the fact that since 1890 the circular 428-day motion has been diminishing its radius in conformity to the requirements of the numerical theory derived from the observations between 1825 and 1890.

In addition to the above, a discussion was presented of 645 observations of the Pole Star made with the Pulkowa Vertical